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freeze again, and others subject to drainage (and a few other causes), often have ice permeating the mass, sometimes in little fine needles, which make the mass worthless, and now and then in little crystals scattered through it. If these crystals are much larger than a pea, and more numerous than one to about every four square inches exposed by a section, the bank is rejected by the Eskimo snow-builder, unless others cannot be found.

The packing of the wind and low temperature are needed to produce the true building-snow, and, in the absence of either one of these conditions, the action of the other seems to be worthless. As to temperature, this is shown by the snow not being good, as judged by the Eskimo, until it is *ik-kee-oo-ad-lo* (very cold) despite the fiercest gales having occurred. It is shown as to the wind by not finding good building material in deep gorges, and other places where the wind cannot get at the snow to pack it down, long after it is perfect in other localities. My information on these points did not come from such observations, however, but directly from Eskimo explanations, and I add these to corroborate them. I do not believe—although I do not positively know—that both wind and low temperature must come together, but both must have happened before the Eskimo will use the snow for building, though possibly the two may be independent in time. When I say the Eskimo will not use it, I mean as a usual thing and in a general way; for in his cheerless country he is often driven to dire expedients, and does many things under a sort of polar protest.

After my detailed description of an Eskimo snow-house in *Science*, and some popular accounts in other periodicals, I learned in several ways (by correspondence and from accounts given me by the editor of *St. Nicholas*) of attempts to reproduce these domiciles in our country having ended in failure. Of course, the main reason of such failures was in the lack of knowledge to construct the igloo, the manual dexterity needed, it being an art which requires no small amount of the early life of an Eskimo to acquire to that perfection we often see among them; yet the builders who failed in their undertakings may console themselves with the fact that it is only in rare cases that the snow will be of the right texture in so low a latitude. The alpine districts, as Mount Washington in the winter, and similar places, might do. Ebierbing (Eskimo Joe, as he was known in the United States), my interpreter, told me that he had built a few igloos in the United States for the edification of curious crowds, but he was only too glad not to see them tumble in and ruin his reputation as well as the house; but, as to living in them, he would never have thought of it.

FRED'K SCHWATKA.

New York City.

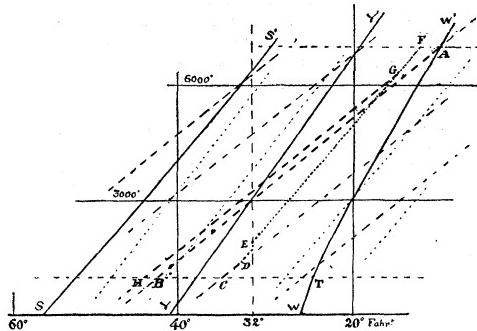
'Chinook winds.'

Dr. Dawson's interesting note on the Chinook winds of the north-west does not fully represent the views on the origin of the *foehn* held by Dr. Hann.

The *foehn* winds, and presumably the Chinook also, are often felt on the leeward side of a range before any rain falls on the windward side: therefore, while the evolution of latent heat by condensing vapor is a true and important cause of the warmth of the *foehn* in the manner indicated by Dr. Dawson, it is not the first or the only cause, and I think it is not the most efficient cause. Dr. Hann has shown

that the first cause of the warmth is the descent of air from the level of the passes and peaks in response to the needs of a low-pressure area on the leeward side of the range; and, as the temperature of the upper air is not greatly lower than that of the surface air in winter (the vertical decrease of temperature in the atmosphere being slow in this season), the descent of the upper air gives it a warmth and dryness that is very abnormal. The *foehn* is indeed, like our north east winds, a current that is propagated backwards; first, the air is withdrawn from the plains in front of the mountains by the approach of a low-pressure area; then the air in the valleys flows out over the plains; next the upper air descends from the passes into the valleys, warming as it falls; finally the air rises on the farther side of the range, clouds form in it, rain falls from it, and it therefore cools slowly in its ascent; but, as soon as the little cloud that crosses the range is dissolved, the air warms rapidly in its descent; and thus the *foehn* is established. Doubtless the last two processes go on together.

I have used the accompanying figure (based on a diagram by Hertz) to illustrate the *foehn* problem:



the full lines represent the variation of mean temperature with altitude for the year (*YY'*), summer (*SS'*) and winter (*WW'*); while the broken lines are ordinary adiabatics, showing the change in temperature of ascending or descending masses of air that are warmer than their dew-point; and the dotted lines are adiabatics for the retarded cooling of masses of air in which vapor is condensing. Now, in winter, when the lower air at a station one thousand feet above the sea, with a temperature of 24° F., (shown at *T*), moves away, and is replaced by air that descends from an elevation of seven thousand feet, where its temperature is 10° (*A*), the latter will reach the ground (*B*) with a temperature about 42°, and a very low relative humidity: it is almost twenty degrees warmer than the air whose place it has taken. The descent must be rapid, or else the air will be much cooled on approaching the cold ground.

A second example shows the action of rain: starting on the farther side of the mountains, with a temperature of 35°, suppose the air ascend five hundred feet from *C* to *D* before any condensation takes place; then, clouds forming and rain falling, further cooling is slow, as shown by the steeper dotted line, *DF'*. Where this line crosses the temperature of 32°, there will be a brief ascent without any cooling, until all the cloud-particles are frozen: this is shown by a short vertical turn at *E*, but the effect is small.

Supposing the air rises to one thousand feet, it will there be cooled to 12° ; then descending, as it passes over the range, it will at first (*FG*) warm as slowly as it cooled, until all the cloud that it carries is dissolved; the rest of the descent has a faster warming (*GH*), and the ground is reached with a temperature of about 43° , or 8° warmer than when the ascent began.

These figures are not precise, as the diagram is rather hastily constructed from Hertz's plate; but they serve to show how much greater a change is produced by the descent of the upper air than by the evolution of latent heat in a transmontane wind. The approach of the line of summer temperature (*SS'*) to parallelism with the adiabatics also illustrates how much fainter the *foehn* must be in summer than in winter.

The following quotation from Espy's "Fourth meteorological report" (1857) is of interest in this connection: "It is known that air, in passing over high mountains, . . . is twenty or thirty degrees warmer than the atmosphere is at the same height over plains, because in passing over them it has the latent caloric in it, just evolved by the condensation of the vapor on the windward side." "Air can never come down from a great height without being very dry when it reaches the surface of the earth." "At the time of this hot south wind, there may be a great rain taking place on the other side of a mountain to the south of the observer, sending its hot air over above, and radiating its abnormal heat down, and even bringing some of the hot air down the slope on the north, which would be felt there as an excessively hot, dry air." He also quotes Lepoy's mention of a warm south-west wind at Fort Simpson, east of the Rocky Mountains in British America, and applies the above explanation to it (pp. 146, 147, 151).

W. M. DAVIS.

Cambridge, Jan. 12.

The claimed wheat and rye hybrid.

There is very slight botanical distinction between the wheat and rye genera, and hence we could scarcely select two genera between which we should more readily expect, *a priori*, a success in hybridization. The question, however, is, Has such a hybridization been effected? Mr. Charles Barnard, who scarcely can speak as a botanist, states in the January *Century*, p. 477, that it has taken place. As one who has carefully studied the published claims, and who has also visited the growing plants upon which the result is claimed, I must beg to dissent. Without opportunity for a careful and thorough examination of the various plants produced, I dare not affirm that such a hybridization has not been effected; yet I do dare affirm that the evidence adduced is insufficient to establish the fact, and is sufficient to establish grave doubts.

What are the facts? The flowers of the Armstrong wheat were treated with pollen from rye. A number of variables were produced from the resulting seed, which, without careful botanical investigation, have been pronounced hybrids. These figures were published in the *Rural New-Yorker* of Aug. 30, 1884.

Lindley distinguishes rye from wheat by its narrow glumes, and constantly twin narrow florets with a membranous abortion between them. In the drawings referred to, the glumes in all the figures are

drawn broader than in the rye. In four of the figures the spikelets are distinctly those of a common wheat. In the fifth figure—the one called by Mr. Carman "a distinct grain, neither wheat nor rye, and as different from either as wheat is from rye, or rye from wheat"—we must look for the hybrid, if at all. This plant, so far as can be indistinctly made out from the figure, has its spikelet solitary on each notch of the axis, with two nearly equal glumes; and the outer pale of each floret has at the top either a notch or angle on each side of the terminal point or awn,—all the distinguishing characters of the genus *Triticum*. It has not the narrow glumes nor the constantly twin narrow florets which are peculiar to rye.

What do these figures resemble, if not rye? Judging by comparison of pictures, his No. 335 is close to the Froment de Saumur; his No. 336, to Froment Pictet; his No. 337, to Froment de Naples; his No. 338, to Froment blanc de Flandre; his No. 339, the supposed hybrid, to Froment de Pologne compact,—all, as figured by Heuze, in the form of the head. I do not mean to say by this that they are these varieties, for the material for judgment does not admit of such close comparison; but I refer to these varieties, and those represented by Mr. Carman's figures, as representing like types of head.

We do not question the attempt at a cross. The variability effected is indication of the influence of a foreign pollen. We can explain the appearances, however, by an hypothesis. Under the stimulus of the rye pollen, atavism has resulted, whereby varieties dormant in the Armstrong wheat have made their appearance; and to those unfamiliar with foreign varieties, whose type appears in the progeny, the seedlings produced seem as if novelties, the unfamiliar Blé de Poland being little known in this country.

The whole subject is, however, too interesting a one to allow to pass without comment such statements as the *Century* article contains, and it is to be hoped that at some time a botanist expert in agricultural botany may have opportunity to investigate a series of these specimens.

E. LEWIS STURTEVANT.

Geneva, N.Y., Jan. 6.

Stepniak's 'Russia under the tzars.'

Will you kindly permit a few words of reply from one of your English readers to M. Woeikof's letter on p. 478 of your issue for Nov. 27, 1885?

We in the old country, who are watching with deep interest the struggle for freedom now going on in Russia, do not attach so much importance as your correspondent seems to think we should, to Stepniak's personal share in the conflict: indeed, we do not even care to inquire about it. The important point for us is the accuracy of the facts he has brought forward. If true, they place the Russian government outside the pale of civilization, and deprive it of all right to appeal to civilized Europe against any act in which the wrath and despair of its subjects may find vent. If false, they can easily be disproved. Stepniak has plainly stated names, dates, and sources of information; his book has now been for a year before the public; and he has reiterated his charges through the leading organ of the English press. If the Russian government is maligned, why does it take no steps to disprove his statements?

But whilst Stepniak's allegations are confirmed by